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First rough draft of a chapter on the development of my work in
Exobiology and in my relationships with NASA and space exploration.

with some notes on eobiology

My earliest firm recollection in this arena is Orson Welles' radio broadcast of H.G. Wells' novel, "War of the Worlds", on October 30, 1938. The event, coming on the heels of the Munich crisis, a jittery public was all too ready to be frightened by a radio dramatization in the then novel newscast format. Inter alii, the astronomy club was one of my extracurricular activities at Stuyvesant High School, and I was of course far too sophisticated to hear the drama as anything but a thriller. In fact for some while I thought that the occasional newsbreaks presented after the broadcast, reassuring people that it was fiction, were part of the setup. The press the next day made me realize that there really had been a widespread panic reaction. This has been analyzed as a social phenomenon, I believe by Cantril.

Speculations about life on Mars had been recurrent features in the popular scientific press in the 1930's, and in books like Percival Lowell's. In 192 , a close opposition, there were announcements of efforts to listen for signals.

During the 1940's Curt Stern gave a seminar at Columbia on Life on Other Worlds, a review of the book by Astronomer Royal Spenser Jones. This would have been a familiar topic of biological speculation extrapolating the vogue about Oparin and theories of the origin of life. I'm sure that I had read everything in sight on the subject as a matter of general but fundamental biological interest, (never with the view of any possibility of experimental realization!). Cosmology and astronomy were pet subjects in part through the writings of people like Jeans and Eddington and from many other sources.

I can recall a spirited conversation with Stevie Tyler (son of the late Professor Albert Tyler, then of Cal Tech) at Woods Hole, in 1954, in which we discussed these conjectures. I had to defend it as an open issue: the idea of canals on Mars never struck me as very credible but there was simply not very much evidence to go on one way or the other.

My serious engagement with these issues began the evening of November 6, 1957 in Calcutta. Esther and I were returning from our Fulbright Lectureship in Australia. Sputnik had been launched on October 4, during our stay in Melbourne. It had of course reverberations around the world; they were possibly deepened by the fact that it was visible to the naked eye during those first days more readily in the southern hemisphere than in the northern so we had the advantage of an early observation. That winter (August, September) was notable for the brilliance of the Aurora Australis.

J.B.S. Haldane had only recently established himself at the Indian Statistical Institute "as a refugee from the US occupation of Britain". (Cf Dromanraju biogr.; Notes & Rec. Royal Soc., 41:211-237 1987 also has some description of the ISI) We had met him at the AAAS meetings in Boston in December 1946: I recall vividly the vials of the wasp *Habrobracon* that Helen Spurway (Mrs. Haldane) carried around in her purse. He had a benevolent understanding of my experiments on genetic recombination; and we had corresponded in the interval. He invited me to visit him in Calcutta and this fit in beautifully with our travel plans

from Australia. We left from Perth, stopped at Singapore and Bangkok before finally arriving in Calcutta on what proved to be a day of lunar eclipse, with religious processions in the crowded streets.

The eclipse was a matter of comment with the Haldanes and their dinner party at the Institute, which included Mrs. Mahalanobis (Dr. M. was away) and a herpetologist, Pamela Robinson. Haldane remarked that this was the 40th anniversary of the "October" revolution: it would be a second coup, after Sputnik, were the Russians to plant some kind of red star on the moon during the eclipse. That red star would have been a thermonuclear demonstration accenting the military prowess signified by Sputnik. A U.S. proposal of that order may be found in the JPL files; but was never seriously contemplated. (But cf S164 file re 12/16/69).. Our discussion lamented how this magnificent scientific opportunity, the beginning of human exploration of space (which excited Haldane no less than it did myself), would be marred by the geopolitical competition, that it would be used for propaganda demonstration rather than scientific inquiry. I began to worry that the scientific integrity of the opportunity was in peril. Further, we might have to protect the moon and other planets from inadvertent radioactive or biological contamination.

Promptly after returning to Madison I began to educate myself more deeply in the general physical and astronomical background of space inquiry, and of rocketry and space travel. Joined Amer. Rocket Society in March 58. In December '57 I composed the first of a series of drafts of a memo. The first of these was entitled Lunar Biology, the second Cosmic Microbiology (Q37). I circulated these to a dozen or so scientific notables, asking their comment on what steps might be taken to avert what I saw as a potential cosmic catastrophe, and inviting broader scientific examination. One of these communications was addressed to Detlev Bronk as the President of the National Academy of Sciences; another to Fred Seitz who was on its governing council. Bronk at that time was also President of The Rockefeller Institute; Seitz was chairman of the physics department at Illinois. The matter was referred to the council of the NAS and on February 8, 1958 it adopted a resolution expressing its concern about planetary contamination and asking ICSU to develop an international framework for the development of policy in that direction. ICSU did establish CETEX, the committee on contamination by extraterrestrial exploration. It met in May 1958, and concluded "there is a real possibility that early experiments might spoil subsequent research", affirming the general principles of caution that I had outlined in my December memoranda. Its conclusions were published in Nature 183: 925-928, April 4, 1959, and provided an important basis of international space policy. In remarkably short time, a sound basis of policy examination had been established.

During the succeeding months U.S. space activities were reorganized under a civilian agency, NASA. The administrator, Hugh Dryden asked the NAS to establish an advisory Space Sciences Board, chaired by Lloyd Berkner. He in turn asked me to chair a panel to study the problems of planetary quarantine, and biological scientific opportunities in space travel. Other panels examined the problems of life support systems and the physiological stresses of the space environment.

In order to minimize transcontinental travel I suggested that two, small parallel groups be established, on the west and east coasts respectively. The first meeting of the "western group on planetary biology" later abbreviated as Westex was held at Stanford on February 21, 1959. Its members included Calvin, Mazia, Stanier, Stent, Weaver -- from Berkeley; Krauskopf, Lederberg, van Niel, Kamphoefner (SRI); Alan Marr -- from UC/Davis; Aaron Novick - from Oregon; Norm Horowitz from CalTech; Hibbs and Davies from NASA/JPL. Harold Urey --

UC/La Jolla; and Carl Sagan a young graduate student at the University of Chicago interested in planetary atmospheres joined subsequent meetings at my invitation. Likewise, under the same sponsorship, the eastern division met on December 19, 1958 at MIT. This was organized by Bruno Rossi, and also embraced Cowie, Doty, Gold, Hartline, Kamen, Levinthal, Luria, MacNichol, Miller, Schmitt, Siström, Townsend, Vishniac, Billings, Freeman and Young.

Eventually I was invited to join the NAS space science board and take part in broader deliberations about space policy. (Meanwhile I had been elected to NAS, and then received the Nobel Prize).

My main preoccupations during this interval, (my early establishment at Stanford University) were for planetary quarantine. The first successful U.S. orbital flight did not ensue until January 31, 1958; and it seemed premature to many to be contemplating lunar, much less planetary, landings. This was precisely my concern: that early approaches to the moon or planets would be crude crash landings most likely to result in contamination, e.g. from radio-isotope electric generators.

In my meetings with various NASA representatives and at JPL, Al Hibbs and others put it to me that I should be undertaking a constructive as well as critical role: why didn't I take a positive part in the development of biological instrumentation for space exploration? Accordingly we established an instrumentation research laboratory in December 1959 with preliminary support from The Rockefeller Foundation, and with a definitive grant from NASA to Stanford in April 1960. I was fortunate to be able to recruit Elliott Levinthal to undertake the directorship of that laboratory and begin our experimental program in "Cytochemical Studies of Planetary Microorganisms". That was an arch title: the only planetary organisms we had were terrestrial ones. But we pondered the methodology by which life might be most efficiently sensed by instruments on a lander on Mars. The implicit assumption was an automated unmanned mission. As will be seen, this continues to be a center of policy controversy. It was also to help lead me into a career in computer science.

My first publication in this field was P77 with Dean B. Cowie, of the Terrestrial Magnetism laboratory of The Carnegie Institution in Washington. He was a physicist who had turned to molecular biology. (Mar 6 1913 - Nov 8 1977). He was a member of EASTEX, but took no active role in space science thereafter. This paper was presented at a symposium in Washington on May 14- 17, 1958 and was published in Science under the title "Moondust" promptly thereafter. I probably met Cowie on January 24, 1958 when he may have visited Madison to give a seminar in his own area of molecular biology: the substitution of Se for S in bacterial proteins. We obviously hit it off very warmly and before long he agreed to vet and coauthor a manuscript of the Moondust paper. I was very glad to have a collaborator whose credibility in physics exceeded my own and who could help keep me well grounded on the very speculative proposals in that paper. We can go into those later.

The speculations in P77 have been rather gratifyingly fulfilled by astronomical observations of interstellar matter and these are captured in the preface to SAM 275. See also my correspondence with Mayo Greenberg. Halley's Comet findings

----- Subsequent history is publically documented in various books

-- Cooper, Ezell, and the technical reports on the Viking mission, though there should be further narrative when all the papers are digested.